GEORGIA INSTITUTE OF TECHNOLOGY — OFFICE OF CONTRACT ADMINISTRATION

PROJECT ADMINISTRATION DATA SHEET

[X] ORIGINAL [ ] REVISION NO. ______

Project No. G-43-607 (R5829-0A0) Co-project with E-24-630 GTRI/~~GIT~~ DATE 9 / 27 / 84

Project Director: Dr. D.E. Chubin School/~~Lab~~ Social Sciences

Sponsor: National Science Foundation

Type Agreement: Grant No. PRA-8413060

Award Period: From 10/1/84 To ~~2/28/85~~ ~~8-31-85~~ 11-30-86 (Performance) 5/31/85 (Reports)

Sponsor Amount: This Change / Total to Date

Estimated: $ 43,254 (Total of both projects) $ 43,254 (Total of both (co-pr

Funded: $ 31,869 (for this project alone) $ 31,869 (for this project alo

Cost Sharing Amount: $ 3,768 Cost Sharing No: G-43- 315 (F5829-0A0)

Title: "Measuring Scientific Output: A Collective Biography Approach"

ADMINISTRATIVE DATA OCA Contact ______

1) Sponsor Technical Contact:

Susan E. Cozzens
Policy Research and Analysis
National Science Foundation
Washington, D.C. 20550

(202)357-7826

2) Sponsor Admin/Contractual Matters:

Richard Hastings
Grants Official
National Science Foundation
Washington, D.C. 20550

(202)357-9626

Defense Priority Rating: ______ Military Security Classification: ______

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See Attached NSF Supplemental Information Sheet for Additional Requirements.

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Equipment: Title vests with GIT

COMMENTS:

This is a continuing grant that is expected to be funded for 15 months. The grant is split between the ISyE school and Social Sciences. Dr. Allan Porter is the Project Director for the co-project in ISyE. Special requirements of this grant state that all prior approvals for requests under Article 2 of the F.L. 200 form must be made through NSF, not by using OPAS.

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SPONSORED PROJECT TERMINATION/CLOSEOUT SHEET

Date 11/19/86

Project No. G-43-607

School/~~Lab~~ Soc. Sci.

Includes Subproject No.(s) E-24-630

Project Director(s) Dr. D. E. Chubin

GTRC / ~~GIT~~

Sponsor National Science Foundation

Title "Measuring Scientific Output: A Collective Biography Approach"

Effective Completion Date: 11/30/86 (Performance) ______ (Reports)

Grant/Contract Closeout Actions Remaining:

- [ ] None
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- [ ] Closing Documents
- [X] Final Report of Inventions - Questionnaire to P.I.
- [ ] Govt. Property Inventory & Related Certificate
- [ ] Classified Material Certificate
- [ ] Other ______

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FORM OCA 69.285

FINAL REPORT

MEASURING SCIENTIFIC OUTPUT: A COLLECTIVE BIOGRAPHY APPROACH

Daryl E. Chubin
Alan L. Porter

Prepared for

National Science Foundation
Washington, D.C. 20550

Under

NSF No. PRA 84-13060

August 1986

GEORGIA INSTITUTE OF TECHNOLOGY
A UNIT OF THE UNIVERSITY SYSTEM OF GEORGIA
SCHOOL OF SOCIAL SCIENCES
ATLANTA, GEORGIA 30332

1986

APPENDIX VII

NATIONAL SCIENCE FOUNDATION
Washington, D.C. 20550

FINAL PROJECT REPORT
NSF FORM 98A

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PART I—PROJECT IDENTIFICATION INFORMATION

1. Institution and Address	2. NSF Program	3. NSF Award Number
Georgia Tech Research Corporation Georgia Institute of Technology Atlanta, Georgia 30332	PRA	PRA 84-13060
	4. Award Period From 10/1/84 To 11/30/86	5. Cumulative Award Amount $90,135

6. Project Title

MEASURING SCIENTIFIC OUTPUT: A COLLECTIVE BIOGRAPHY APPROACH

PART II—SUMMARY OF COMPLETED PROJECT *(FOR PUBLIC USE)*

The purpose of the project was to explore novel approaches to measuring scientific output. Phase 1 utilized bibliometrics to analyze the outputs of the 1974 and 1984 cohorts of Sloan Foundation Fellowship recipients in chemistry (Interim Report, February 1985). Phase 2, reported here, features a principal output of science--"progress"--and its characterization in "third-party" and "first-person" texts. Our content analysis focuses on 200 reference letters written in behalf of candidates for (a) Sloan Fellowships in chemistry, neuroscience, mathematics, physics, and economics, and (b) Lawrence Awards for contributions to "atomic energy research." Interviews with 25 scientists and engineers further suggest that the concept of progress has referents that differ by field, specialty, and local research program. Three categories of exposition derive from the evaluations of referees, while eight themes are identified through the interviews. Translating the "functional grammar" of the researcher into policy-useful data for the decisionmaker is the challenge to the social analyst. We conclude that methodological, terminological, and interpretive insights accrue from the analysis of scientific texts that augment conventional quantitative measures of output.

PART III—TECHNICAL INFORMATION *(FOR PROGRAM MANAGEMENT USES)*

1. ITEM *(Check appropriate blocks)*	NONE	ATTACHED	PREVIOUSLY FURNISHED	TO BE FURNISHED SEPARATELY TO PROG[...] Check (√)	Approx. Da[...]
a. Abstracts of Theses	X				
b. Publication Citations		X		X	10/86
c. Data on Scientific Collaborators	X				
d. Information on Inventions	X				
e. Technical Description of Project and Results		X			
f. Other *(specify)*					

2. Principal Investigator/Project Director Name *(Typed)*	3. Principal Investigator/Project Director Signature	4. Date
Daryl E. Chubin		8/6/86

NSF Form 98A (3-83) Supersedes All Previous Editions — Form Approved OMB No. 3145

MEASURING SCIENTIFIC OUTPUT:
A COLLECTIVE BIOGRAPHY APPROACH

by

Daryl E. Chubin, Principal Investigator

Alan L. Porter, Co-Principal Investigator

(with the assistance of Ashish K. Sareen)

Schools of Social Sciences and Industrial & Systems Engineering
Georgia Institute of Technology
Atlanta, Georgia 30332

FINAL REPORT

to the Division of Policy Research and Analysis
National Science Foundation

Grant No. PRA 84-13060

Susan E. Cozzens, Project Monitor

August 1986

EXECUTIVE SUMMARY

The purpose of this project was to explore novel approaches to measuring scientific output. Collective biography defines a study population that shares a key experience or characteristic. Analysis of this population yields explanations of their aggregate performance and the context in which it occurs. In Phase 1, we focused on the 1974 and 1984 cohorts of Sloan Foundation Fellowship recipients in chemistry. A bibliometric analysis indicated the need for comparisons with three other Sloan fields--neuroscience, mathematics, and physics--and for qualitative data on the evaluation of scientific talent, process, and output (Interim Report, February 1985).

Our mandate in Phase 2 was to examine the meaning of the chief output of science--"progress." How do analysts identify progress, characterize it, and use it to evaluate research programs? Our response to these questions was to ask scientists as participants in specialized research communities how they recognize and describe advances. A review of literature suggested this has seldom been done systematically.

Our tool thus became content analysis of scientific "texts." Having reviewed the scholarly journal literature written for specialists in Phase 1, we concentrated on various "third-party" and "first-person" texts. The chief data source of the former were reference letters written in behalf of candidates for Sloan Fellowships and Lawrence Awards. We reviewed over 200 of these letters, with the identities of both the referees and the candi-

dates masked. In addition, we conducted phone interviews with ten of the 14 Sloan Program Committee panelists who selected winners in 1984, and examined 14 profiles of Nobel laureate chemists published in SCIENCE. Three categories of exposition derived from the sample of Sloan letters and applied to the Lawrence letter sample: Techniques/methods, Schemes/solutions, and Observations/experiments. The distributions vary by field. The Lawrence reference letters, aggregated by nominee, prompted detailed analysis within field. A combination of contributions to "atomic energy research" (broadly defined) seem more prevalent here. A clear emphasis on Technique and Observation emerges.

The centerpiece of Phase 2, however, was the oral testimony of 25 scientists and engineers. These in-person interviews were loosely structured to develop the "functional grammars" that specialists trained across the spectrum of natural science, social science, and engineering disciplines employ when describing scientific advances. They emphasize several notions. Progress comes in two varieties, the familiar "incremental" and the "eureka" kind that "others" experience. In all, eight clustered themes-proxies or components--of progress are described: funding, literature-jargon-theory, methodology and instrumentation, commercialization, and ad hominem ("people," not "things"). Interpreting these themes raises other issues: the "adventurous" research style, the skill mix demands of problems that engender interdisciplinary collaboration, the inertia of well-institutionalized "research trails," and the blurred perspectives on progress that practitioners currently at the research front suffer.

These findings lead to methodological injunctions, especially for the decisionmaker. Is there an evaluative component in the analysis of scientific texts that can augment conventional quantitative indicators of scientific performance? We think so. But there are caveats. Scientific texts can be used to understand what scientists mean by progress. These texts reduce the "gap of competence" that exists between technical specialists and social analysts. Science policy now relies in part on social assessments of scientists' performance and outputs. The challenge, then, is to translate scientists' referents for and criteria of progress into usable categories for evaluation and decisionmaking purposes.

We conclude with five generalizations on how conceptual data facilitates the understanding of scientific progress and two recommendations on the use of such data for measuring outputs of science.

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A COLLECTIVE BIOGRAPHY APPROACH

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INTRODUCTION TO PHASE TWO

No concept is more central in science than "progress." Scientists invoke it as an article of faith, their most important product, and justification for their continued support. As Kuhn (1970: 179) observed in THE STRUCTURE OF SCIENTIFIC REVOLUTIONS, "... scientific progress is not quite what we had taken it to be. But... a sort of progress will inevitably characterize the scientific enterprise as long as such an enterprise survives."

The problem is this: what does "progress" mean? This project came to address the meaning of progress after (1) examining the literature of young investigators recognized as contributing to research progress, (2) conducting phone interviews with a sample of 1984 Sloan Foundation Basic Research Fellows, and (3) concluding in our interim project report (Chubin and Porter, 1985) that specialization within the sciences creates vexing methodological, terminological, and interpretive problems for the social analyst. Phase 2 of the project was devoted to exploring empirically these problems.

Though "scientific progress" may be recited like a mantra by researchers, it must have referents. Measuring the reality that corresponds to the rhetoric was a task one step removed from our initial purpose: how to measure "science output." If progress is an output, then how do we analysts identify it, characterize it, and use it to understand science? Our response to these questions was to ask scientists qua participants in specialized communities with active research programs. Methodologically, our approach, though straightforward, by no means assured us that scientists immersed in research would be willing to reflect on the experience. Even if they were willing, would they be able to articulate to non-specialists like ourselves what they saw as and meant by "progress"? Terminologically, would their words refer to other concepts that analysts take as synonymous with, or symptomatic of, progress, e.g., quality, productivity, and discovery? Would their use of progress fit with our familiar notions of theory, experiment, serendipity, etc.?

The concept of progress denotes a state or product but it connotes a dynamic process. Social analysts of science thus describe progress as a snapshot of a juncture, episode, or milestone in the social production of knowledge. For example, the act of choosing a problem for investigation, consensus that a replication of an experiment has occurred, the allocation of credit to members of a team, and the transmission of tacit knowledge to neophyte researchers (a kind of modeling) are all signposts on the avenue Scientific Progress. The bibliometrician, ethnographer, and discourse analyst would each focus on different aspects of the scientist's constructions of, or claims to, progress.

We have examined various types of text: the primary (or scholarly) journal literature written for specialists, the pedagogical memoir-honorific essay that reconstructs history, the letter of reference that extols the promise of early research accomplishment, and the first-person oral testimony that places progress into its current research context. In reading these texts, we have not been concerned with the origins of ideas, or styles of exposition per se. We have become sensitive, however, to scientists' use of language, especially metaphor and imagery, in describing research experiences.

Above all, we have noted the frustration of specialists trying to convey to non-specialists the "intellectual" prerequisites for progress. This contrasts somewhat with their facility in describing the "instrumental" conditions of progress in research. The contrast indicates a continuum of progress ranging from puzzles "internal" to a science that create new intellectual challenges to "external" problem solutions (beyond the domain of a particular research community) that may eventually enhance the quality of life. The long-range implications of research do not immediately spring to the mind of a scientist in the written accounts of his/her work. To a non-cognoscente, the accounts are usually modest and not very intelligible.

Progress, then, is neither "basic" nor "applied"; it is rather a way that researchers establish and appeal to a standard. This standard becomes a proxy for "the advancement of knowledge," "the research breakthrough," and everything innovative, novel, and even revolutionary in science. In the words of scientists--both written and spoken, there are numerous "proxies" for progress. These reach thresholds of distillation, dilution, and popularization. Scientists vacillate among them, we suspect, because they are not trained to speak with non-specialists; many are uncomfortable in doing so. Some resent the scrutiny implied by our interest; others dichotomize their audience as fellow specialists and the press. Social analysts as interviewers fall somewhere in between: we can ask some of the right questions, but don't readily understand the answers.

It is easy for us to assert that thresholds of understanding are either respected or violated by our interviewee-scientists. We may call this "negotiation" or "vocabularies of justification" in our post-game analysis, but during the contest of data-collection, we realize the contradiction: simultaneously we torment the scientists with our ignorance but flatter them with our interrogation (see Appendix A). What does this approach yield?

An unstructured interview allows the scientist to warm to the subject of progress. For some, it is an opportunity to think aloud ("I never thought of it that way"). For others, it is a challenge to make transparent that which the specialist usually takes for granted ("everybody knows that"). The degree of patience and responsiveness recorded in our interview transcripts is an unobtrusive measure of how novel or moribund the subject of progress, posed by "outsiders," struck the scientist. The range on this measure emerges from the 334 pages of transcription. Perhaps had we spent more than 16 hours with 25 scientists and engineers, we might have produced a generalizable trend. For the exploratory study it was intended to be, more interviews were not justified. We terminated Phase 2 and took stock.

So what have we learned? How might we best present it? The words of our sampled scientists constitute the centerpiece of this Final Report. We have organized their words thematically under the proxies that they seem to employ. Preceding this presentation, however, are two distinct sections. One is a summary analysis of the words found in a sample of letters of recommendation written for Sloan Foundation Fellowship and Lawrence Award candidates. The second, and more immediate, section is a brief bibliographic essay which highlights what little is known empirically about how scientists identify and measure their "most important product"--progress.

I. BIBLIOGRAPHIC ESSAY

Systematic study of scientific progress seldom goes beyond the bounds, as Laudan (1977: 2) observes, of "specialized disciplinary monopolies." Not until Kuhn's THE STRUCTURE OF SCIENTIFIC REVOLUTIONS (1962) did the boundaries of "disciplinary monopolies" begin to blur. The long tradition of debate among historians and philosophers of science about the nature of scientific knowledge and its advance, eschewing recourse to scientific practice, was replaced by a convergence of the history, philosophy, and sociology of science on the "research community" as a social and cognitive unit. Toulmin (1977) termed the convergence an evolution from "form to function." Such evolution itself is hailed as a type of progress, a process that characterizes longer ("normal") periods than Kuhn's episodes of anomaly-crisis-revolution in science.

Evolutionary-revolutionary models feature an inexorable movement of idea generation and recombination; this movement signals intellectual change. Whether we label such changes "research programs" (that are definitionally "progressive," according to Lakatos), "paradigm shifts," or "research traditions," we are demarcating change in science. But again, what kind of change? By looking back we seek guideposts. But can we use concepts of retrospection for looking forward? Does analysis of the products of science, and commentary by the producers and consumers themselves, aid in illuminating the process, or conditions, of progress? We think so.

One basis for our optimism is an historical analysis by Deutsch et al. (1971) tracing 62 advances in social science since 1900. The authors' definition of "advance" is instructive for our purpose: it had to involve "a new perception of relationships" or "new operations." In addition, "it should have proved fruitful in producing a substantial impact that led to further knowledge" (1971: 450). Classifying advances as "nonquantitative results," "applications to quantitative problems explicit and/or implied," or "quantitative findings explicit," Deutsch et al. conclude that the

> long-standing quarrel about whether to emphasize theory, methodology, or empirical results seems ill-conceived and obsolete. All three seem to form part of one production cycle of knowledge, and substantial advances in any one of these three phases are likely to lead to advances in the other two" (1971: 456).

Other parameters of advance in social science reported by Deutsch et al. include stimulation of the work by a practical demand, its interdisciplinary conduct, and its ultimate impact or application to social practice and broad fields of scientific activity within an average of ten years following the advance. The criterion of conversion from basic knowledge to application (in the form of invention, diagnosis, therapy, etc.) is apparent in the ideology of the Project Hindsight-TRACES reports of the 1960s, and in the enumeration of biomedical advances into the '70s (Comroe and Dripps, 1976; Comroe, 1978). The social use of scientific knowledge clearly drives these analyses, underscoring the notions of "cumulation" and "progress."

Similarly, Weinberg's (1967) "criteria of scientific choice" offer three baselines for measuring progress--impacts internal to a discipline, impacts within science but between disciplines, and impacts on society. Hindsight and growing conceptual sophistication among social analysts of science to explain the empirical efforts of the previous two decades in gauging progress against these three baselines lead to observations that "The locus of knowledge is not the written word or symbol but the community of expert practitioners" (Collins, 1985: 159-160). Scientific work becomes "the organization of persuasion through literary inscription" (Latour and Woolgar, 1979: 88). This fixes the focus on "scientists' talk and texts" (Knorr-Cetina and Mulkay, 1983: 10)--accounts that provide indicators of scientific practice. For the discourse analyst, no other focus matters (Mulkay and Gilbert, 1984).

Practice without context and linkage to the research and patronage communities outside the local laboratory, however, is analytically vacuous. For what distinguishes the research community is not local interpretations of progress, but interpretations that are shared by those with a stake in the advance of that science. Indeed, the communication among those actors who reside in different communities--competing claims of progress--involves the "transfer of knowledge from one expository genre to another" (Cloitre and Shinn, 1985: 32).

This transfer of knowledge is most analagous to what the social analyst faces when querying the scientist about his/her perceptions of progress. Analysts lack a technical frame of reference, or what Hagerstrand (1985: ix) calls "the 'taken-for-granted' culture of the separate fields of learning... elaborated through a mute agreement between those who have been trained in a certain tradition." The social analyst therefore is much like the consumer of "popularizations." For instance, from their examination of various expository texts in fluid physics, physical chemistry, and mechanics, Cloitre and Shinn (1985: 33) reduced 80 percent of the themes encountered to five referents. These, like those noted by Deutsch et al. above, function as proposed parameters of progress:

(a) Phenomenon: the physical world in terms of matter and its relations;

(b) Experimental protocol: methodology, technique, and the operation of instruments;

(c) Research in neighboring fields: familiar protocol, interpretive representation, or focus;

(d) Historical accounts of former research;

(e) Industry: technology and general economic factors.

In addition to referent, Cloitre and Shinn characterize scientific exposition in terms of imagery (graphs, metaphors) and argument (quantitative, intuitive). They find that texts prepared with different audiences in mind (specialist, inter-specialist, pedagogy, popular) utilize different categories of expository device. For example, specialist texts are high in referents to experimental protocol, in graphic imagery, and in quantitative argument. Popularization, in contrast, is high in historical referent, metaphorical imagery, and qualitative argument (1985: 34).

Do scientists, however, talk the way they write? And do they write the way they act? Social analysts have been incredulous about scientists' professed consistency among their talk, texts, and practice. Further, does this lack of consistency among accounts--written, oral, and in situ observation--reflect differences among scientists, among practitioners, among analysts, or some combination thereof? No definitive answers are forthcoming, but sufficient (and often eloquent) doubt has been cast to warrant inquiry into scientists' discourse (e.g., Latour, 1983; Lynch, 1985; Mulkay et al., 1983; Mulkay, 1984).

To address differences in "the identification of progress" across fields, an international colloquium was organized by the European Science Foundation in 1983. Ten fields were represented: physics, mathematics, biology, medicine, sociology, linguistics, art history, history, economics, and ecology. According to the published proceedings (Hagerstrand, 1985), a chief objective of the colloquium was to identify criteria of advancement, especially agreement and disagreement across disciplines as to what constitutes "advance" as well as its promotion and inhibition.

The commentaries in four of the ten fields--mathematics, biology, linguistics, and ecology--are especially relevant to our search for parameters of progress. Progress in mathematics, for example, is characterized by "a continuous application of standard methods interspersed with spectacular breakthroughs when new concepts and problems suddenly appear" (Atiyah, 1985: 28). In such a theoretical science, a surprising result is often the "counter-example" that indicates "no further progress is possible in a certain direction" (Atiyah, 1985: 29). In terms of Weinberg's typology, advances in mathematics tend to derive from its applications, or "in response to requirements from outside [the discipline], such as information theory, control theory, or epidemiology" (Atiyah, 1985: 31).

In microbiology, several referents for progress can be cited:

1. new knowledge which has given rise to a fundamental "problematique" and alternative theories of explanation, e.g., the origin of procaryotic v. eucaryotic cells;

2. the unfruitful and unsuccessful discussion of the bacterial species concept;

3. the introduction of numerical taxonomy which, 25 years later, seems to have exhausted both its theoretical capacity and its applications in biology; and

4. understanding cell evolution through research on plasmids and viruses (Gyllenberg, 1985: 44-45).

As the linguist Dik (1985: 117) explains, progress is defined relative to the "object of inquiry" and the "aims and goals" of research in a field. As Slobodkin (1985: 191) reminds us, "Most published studies conclude with an explanation of what is needed in the future. This permits tentative definition of the more obvious breakthroughs." In ecology (Slobodkin's discipline), there is a desperate need for data manipulation capacity

> to serve as baseline information to aid in the assessment of future environmental impacts.... The relatively simple case of the effect of one nuclear power plant, Indian Point, New York, on one species of fish, the striped bass, has generated several hundred pounds of information-laden paper. Methods of easy access to such data would almost certainly constitute a major breakthrough (Slobodkin, 1985: 192).

These examples suggest that common categories of comparison across disciplines, such as cumulation, integration, and innovation, may fail to capture either the referents, imagery, or arguments associated with progress in science. Clearly, new empirical observations drive progress in ecology, whereas theoretical formulations prove, simplify, or synthesize progress in mathematics. Goals structure expectations so that the criteria prevalent in a discipline or peculiar to an interdisciplinary "problematique" imply a "wish list" of means that would permit either acceleration in incremental scientific progress or the consummate "quantum leap."

Thus, progress in the study (measurement, understanding) of progress in science, Dik (1985: 123) would say, is a problem in "functional grammar":

> how communicative verbal interaction is made possible by the possession of linguistic knowledge, and, con-

> versely, to what extent the organization of natural language is determined by conditions imposed on its communicative use.

Functional grammar brings us full circle to the culture of a science, specifically, the paradigms and tacit knowledge which bind practitioners in an ongoing evaluation of what they take to be progressive. Recall that at the outset we mentioned "progress" as a prominent concern within the domain of philosophers, their disdain for scientific practice as an empirical arbiter notwithstanding. To the foundationalist philosopher of science, progress is the product of rational discourse--propositional logic and proper theory. But alas, the philosopher prescribes and the scientist ignores.

What constitutes "good" theory and research, like progress, is a pragmatic judgment. If we detach an obsession with rationality from the (anti-foundationalist) philosopher's assumption of progress, then we arrive at Laudan's (1977: 5-7) substitution of "research tradition" for theory and an emphasis on "problem solving effectiveness" and theory choice. Progress becomes embedded in practice. Recourse to the scientist's research community overrides the categories we might impose. Instead, we learn to hear the "functional grammar" that the practitioners employ, and try to translate their discourse into the grammar of social analysis (for an example, see Pinch, 1985).

II. THIRD-PARTY TEXT AS EVALUATION OF SCIENTIFIC PROGRESS

Over the course of our investigation of "scientific progress," we sought to tap several sources wherein one scientist comments on the work of another. Such peer evaluation is a reputed cornerstone of advance. Four sources of such evaluation are reported here:

* Sloan Fellowship panelists (phone interviews)
* Sloan Fellowship reference letters
* Lawrence Award reference letters
* Nobel Prize profiles in SCIENCE.

A. Sloan Fellowship Panelists

We interviewed ten of the 15 Sloan Foundation Program Committee panelists (for the 1984 competition) concerning their selection of Fellows: what do they look for in candidates and what are appropriate measures of scientific progress? Many panelists indicated that "independence" was a key factor in distinguishing the promise of a young researcher (within five years post-PhD). As one panelist put it, "emphasis is on exceptional promise rather than great achievement."

The other common criterion is "originality," as evidenced by phrases such as:

> "real originality--a new kind of thought or approach" [Chemist]
>
> "people doing something different--in the questions they ask as well as how they go about answering them ... making a difference in how people look at the subject" [Economist]
>
> "departure from conventional work that... had an impact" [Physicist]
>
> "effective originality" [Neuroscientist]
>
> "innovative and exciting" [Mathematician].

As for measures of scientific progress per se, the sentiment of the senior scientists on the Sloan Program Committee can be summarized in the words of a Physics panelist: "I am skeptical of any formula to gauge scientific progress." An Economics panelist intoned, "scientific progress is not easily reflected in quantitative measures. The c.v. indicates some level of productivity--a necessary, not sufficient, condition." What, then, do these evaluators trust? Peer judgment. In the words of a Chemistry panelist: "The n most important ideas in chemistry in the past k years--everybody grasps and appreciates these and how they impact on chemistry." But do written third-party texts systematize and elaborate these truisms and perceptions?

B. Sloan Fellowship Reference Letters

Near the end of Phase 1 we requested access to the Sloan Foundation's files on fellowship candidates. The data we sought were the nominating/supporting letters of recommendation written in behalf of the candidates. These are submitted or solicited prior to deliberations by the Program Committee to select fellowship recipients.

Therefore, on 20 February 1985 (see Appendix F of the 1985 Interim Report for the complete letter), we sent a letter seeking "permission to have Foundation personnel copy the text of your [reference] letter, masking all identification of both you and the nominee. The Foundation will send this request to you without disclosing the mailing list to us."

Sloan agreed to sample randomly four of the fields in which the Foundation awards fellowships--Neuroscience, (applied) Mathematics, Chemistry, and Physics (and astronomy)--producing the names of six 1984 Fellows in each and requesting of each referee that a masked copy of his/her letter be forwarded to us for aggregate analysis.

This procedure resulted in 66 letters written 1979-83 (since nominees can be carried over from year to year). Table 1 summarizes the distribution of content, by field, found in the reference letters. The tripartite classification is a summary measure which assigns the referee's remarks to ONE category. In the case where a referee confined remarks to the candidate's personal characteristics without commenting on the achievement of the work, the type of contribution cited was classified as "Not Stated."

Differences across fields, as expected, based on the bibliographic essay, are indeed evident in Table 1. Observations/experiments (code O below) are the modal contribution cited in Neuroscience, new Techniques/methods (T) in Chemistry, new Schemes/solutions (S) in Mathematics, and no one category of contribution dominates in Physics. Besides the intriguing pattern of these numbers, the words are also suggestive. Excerpts, by field, follow.

Neuroscience:

> "... devised a scheme whereby an irregularity... was propagated... whose statistical properties could be uncoupled..." (S)

Table 1. Sloan Fellowship Reference Letter Emphases

Field	N of Letters	T	S	O	NS	Perc T	S
Neuroscience	18	6	2	10		33.3	11.
Chemistry	19	11	1	4	3	68.8	6.
Physics/Astronomy	15	5	5	4	1	35.7	35.
Applied Mathematics	14	0	14	0	0	0.	100.
Total						35.5	35.

Note: T stands for new technique/method;
S stands for new scheme/solution;
O stands for observations/experiments;
NS stands for not stated (or miscellaneous other).
Tallies are based on content analysis of the reference le
In all cases a single category is used. **Percent** excludes Not Sta
Letters are not identified by nominee. Six nominees were inclu
each category and all received fellowships.

"... developed a binding assay that demonstrated ..." (S)

"... established immunocytochemical methods..." (T)

"... originators of this powerful technique... extended this approach to the analysis..." (T)

"... continually examining and modifying and developing new techniques..." (T)

"... made several significant observations..." (U)

"... produced valuable and in some cases startling results..." (U)

"... one of the first to identify monoclonal antibodies against identified neurons." (S)

Chemistry:

"... first successful observation of surface-enhanced Raman scattering from a monolayer of pyridine..." (U)

"... developing spectroscopic methods for assigning organic structures..." (T)

"... elegant two-dimensional homonuclear spin correlating measurements..." (U)

"... an elegant and non-routine combination of new NMR and mass spectroscopic techniques." (T)

"... elucidating the structure of transplantation antigens." (S)

"... developed an entirely new quantum number scaling approach to understanding state-to-state inelastic processes." (T)

"... developed a density matrix method for analyzing the orientation dependence..." (T)

"... very efficient, elegant, and novel approach ..." (T)

"His most significant contribution is the development and application of internal state scaling theories." (S)

Mathematics:

"... describing the tame fundamental group of a curve in algebraic terms..." (S)

"... absolutely fundamental insight into what's what with special linear systems." (S)

"... found generalizations of Lefschetz-Barth theorems... provided a key insight for recent solutions to Zariski's problem... maded several important contributions to the modern understanding of intersection theory." (S)

"... he distinguished himself by providing an elegant new proof of the corona theorem." (S)

"... gives a beautiful refinement of the reiteration theorems... [and a] new counterexample to a conjecture of..." (S)

"... disproves a conjecture of... [and] gives a counterexample to a proposed invertibility criterion... which has been around for about ten years." (S)

Physics:

"... formal and practical techniques for handling the quantum many-body problem through functional integrals..." (T)

"... has set a standard of using all data to their fullest extent..." (U)

"... is an effective user of every kind of telescope system." (U)

"... has used many instruments... is the most careful and thorough observer I know.... very persistent in the quest for good data in the face of adversity .... is also excellent at statistical analysis." (U)

"... developed novel scaling theories for rotationally inelastic cross sections..." (S)

"... developing... a new approach to scattering processes based on group theory." (T)

If the reference letter is considered an inter-specialist document--not as esoteric as the technical article, but written to persuade the near-specialist--then few proxies for progress generalize across the four Sloan Fellowship fields. The only recurring notions that are shared by two or more fields are novelty/priority, "development," and "elegance." In Mathematics, "insight" comes from "refinement" and "counter-example" (as noted by Atiyah in the Bibliographic Essay. Note, too, that with the removal of Mathematics, the universal "S" category, the distribution by T, S, and U in Table 1 changes dramatically.)

Since referees tend to couch letters of recommendation in terms of "the person" instead of "the work," their claims are low on imagery and higher on "intuitive argument." And finally, the research of Sloan Fellowship candidates is still seen more as scientific promise than scientific deed.

C. Lawrence Award Reference Letters

At about the same time that we sought access to the Sloan reference letters, we were granted access to a set of reference letters submitted on behalf of candidates for Lawrence Awards. The E.O. Lawrence Memorial Awards are granted by the U.S. Department of Energy "in recognition of especially meritorious contributions to the development, use, or control of atomic energy (broadly interpreted) in areas of science related to atomic energy, including medicine and engineering." Eligible individuals should "presently be early in their chosen careers (once specified as under 45, but restated to comply with age discrimination concerns), and show exceptional promise of future development." In comparison with the Sloan Fellowships, however, the Lawrence Awards have a record of candidates' deeds instead of promise on which to base their selections.

In late March, 1985, we received a sample of nomination and reference letters for eight winners and 14 non-winners of Lawrence Awards through 1984. The 141 letters were written from 1974 to 1982. Although letters could be carried in an individual's file for up to three years, each year's screening panels acted independently of prior years' panels. No awards were made in 1978 and 1979, so the letters we secured pertained to nominees in 1977, 1980, and/or 1981. Identities of nominees and nominators were masked. We were informed as to which letters referred to a given nominee, and whether that nominee had won the Award yet.

The sample was selected to ensure comparison with the Sloan Fellows. Most (17 of 22) of the nominees fell in the "Chemistry/Metallurgy" or "Life Sciences" categories, allowing some comparison with the "Chemistry" and "Neuroscience" Sloan categories. Two other nominees fit into "Physics"; two were in "National Security"; and one was nominated across categories (a statistician).

Just as with the Sloan letters, we tried to categorize each letter by its single dominant emphasis: (O)bservations/experiments, new (T)echniques/methods, or new (S)chemes/solutions. Assignment to one of these three categories was achieved for 107 of the 141 letters. In the other cases, mutually exclusive assignment was impossible. Where two categories were highlighted in the letter, one-half was scored in each (17 cases). Where one category was dominant, but one or more others was distinctly cited as a significant contribution to scientific progress, one was credited to the primary category and one-half to any other (17 cases).

Results are summarized in Table 2. To begin, we note the extent of agreement among the referees for a given nominee. In general, clear-cut patterns emerge, yet the letters do not cluster in a single category. Where differences in categorization emerge (e.g., nominee A), we find referees focusing on different aspects of the individual's work. A few examples will suffice (given the generous number of Sloan letter excerpts given above) to illustrate the T, O, and S categories.

Table 2. Lawrence Award Reference Letter Emphases

Field	Winner?	Nominee	N of Letters	T	S	O	NS	Perce T	S
Life Sciences	no	A	5	0.5	1.5	3.5	1		
	no	B	3	1	2.5	0	0		
	no	C	3	0	0	3	0		
		Sum						12.5	33.
	yes	D	18	4.5	0	11.5	3		
	yes	E	4	2.5	0.5	3.5	0		
	yes	F	15	0	1	10	4		
		Sum						20.9	4.
Chemistry	no	G	1	1	0	0	0		
	no	H	7	0	0	1	6		
	no	I	8	4.5	5	1	0		
	no	J	3	0	0	2	1		
	no	K	4	2	1	0.5	0.5		
	no	L	9	4.5	0	4.5	0		
		Sum						44.4	22.
	yes	M	9	3	1	5.5	0		
	yes	N	4	4	0	0	0		
	yes	O	5	4	0.5	2.5	0		
	yes	P	4	2	0	2	0		
	yes	Q	10	10	0	0	0		
		Sum						70.8	4.
Physics	no	R	6	0	5	0	1		
	no	S	1	1	0	0	0		
		Sum						16.7	83.
Other	no	T	6	6	0	0	0		
	no	U	5	5	0	0	0		
	no	V	11	10	0	0	1		
		Sum						100.0	0.
Total								**49.6**	**13.**

Note: T stands for new technique/method:
S stands for new scheme/solution;
O stands for observations/experiments:
NS stands for not stated (or miscellaneous other).
Tallies are based on content analysis of the reference let
Wherever possible a single category is used. When two categorie
equally prominent, each is credited 1/2 point. When one catego
primary, but another is important, the primary one receives 1 p
the secondary one, 1/2 point. **Percent** excludes Not Stated's.

"... outstanding technical contribution to centrifuge separation processes of vital importance to our country's welfare." (T)

"____ is the outstanding practioner [sic] of neutron diffraction as a crystallographic structural tool in the U.S.--and probably the world--today." (T)

"____ has made outstanding contributions to the understanding and quantification of the movement of radioactive contaminants through soils and thus their availability to man." (O)

"In the field of single crystal neutron diffraction, a plethora of fascinating new information on metal hydrides, carbenes, borohydrides, and other organometallics has come from the incisive (and well-known) studies of ____ and his associates. ____ is undoubtedly the single leading figure in this field today and a major reason why neutron studies have impacted so heavily on contemporary inorganic chemistry." (O)

"____ is certainly the leading theorist of his generation, and one of the major theorists of our time. He has made significant contributions to all areas of particle theory..." (S)

Entries in the residual "Not Stated" category emphasize personal work habits, teaching contributions, project management, and/or committee service. For instance,

"____ has been three times selected to serve on NRC study committees... He has demonstrated not only a very high level of scientific and technical competence, but also a commendable skill in being able to bring this competence to bear on sensitive issues of public concern." (NS)

Such commendations do not constitute major criteria for the Awards. Rather, panelists value evidence of direct contribution to the mainstream of research advances. This central criterion is consistent subscribed to in all our third-party texts.

After ALP read the 141 Lawrence letters, he surmised that many scientific contributions combine two or more of the T, O, S dimensions. A recoding by DEC of a random sample of ten letters yielded a reliability of four exact agreements, five partial (one category of a combination in common) agreements, and one disagreement. Even with these differences in judgment, we consider our results defensible.

The profiles of the scientists in Table 2, excluding those failing to accrue at least three categorical "points" (nominees G, J, and S) or pattern (nominee K), distribute as follows:

(S)cheme/solution (theory) dominant	n= 2
(O)bservation/experiment (empirical findings) dominant	5
(T)echnique/method development dominant	5
T/O combined contributions	5
T/S combined contributions	1

T and O seem especially complementary in this sample of scientific evaluation. Developing or refining a technique fits so naturally with its application to important problems (that probably stimulated technique development in the first place). Missing from this tally (but prominent in the Nobel reports described below) is the conjoint effort of running experiments and generating interpretive schemes (e.g., models) to explain the results.

To exemplify the sense of interplay, look back to the second quotes above, respectively, illustrating T and O. They pertain to the same nominee. Another of his referees emphasizes the T/O combination:

> Over the past ten years, ____ has been the driving force behind the appearance of a large and important body of structural data obtained by neutron diffraction. He first developed a unique facility, then developed his own program of research about this instrumentation, and has now made his instrumentation and expertise into a nationally used and recognized facility. (T/O)

The rarer combination of T and S is revealed in this example (with O an implicit intermediary):

> "... the work combines the use of a new and difficult technique to the solution of a major medical and scientific problem--the structural basis of anti-body function." (T/S)

One of the concerns in any program of scientific awards is the extent to which whom one knows counts more than what one does. Reading the Lawrence letters leaves the impression that while networking may influence, it does not determine, these awards. Consider two candidates in Chemistry. One, located at a government lab, received seven supporting letters, including compliments from a politician, testimony to personal traits and contributions by co-workers, and observations from professionals whose only contact with the nominee was through his chairmanship of a series of conferences. We classified six of the seven letters in the "Not Stated" category. The following excerpt was typical:

> I have known ____ for about eight or nine years and have worked with him in various capacities during that time. In every case I have found him to be a very hard worker... One of the main characteristics of ____ is that of sincerity and thoughtfulness. He is always a gentleman as well as a scholar.

Contrast this case with that of another chemist (apparently from an elite university) with nine letters (authored on Berkeley, Caltech, Harvard, MIT, and Stanford letterheads). These were classified as O (n=3), S (1), T (1), and T/O (4). Several of the letters echoed the following sentiment:

> To my knowledge there is no member of the chemical physics community who would select an individual other than ____ as the leading figure in the field.

The nominee in this case, a Lawrence Award winner, might enjoy a relative visibility advantage over the so-called gentleman and scholar above (a non-winner through 1984), but clearly the former's peers join in acknowledging his contributions and not just his institutional affiliation.

A final comparison in Table 2 is between winners and non-winners. Within both the Life Sciences and Chemistry, Schemes/Solutions (theorizing) is not associated with (predictive of?) the award. While the number of nominations is too small to represent the respective research communities, there is an emphasis on observation among the award-winning life scientists and on technique advancement among the winning chemists.

Any apparent distinctions between winners and non-winners appear less pronounced than the differences between Life Sciences and Chemistry. (That is certainly no surprise given the very select samples in both cases.) Overall, there is strong balance between new Techniques and important Observations. Although analyzing a sample of letters from a representative cross-section of scientific fields would be illuminating, at least for the Life Sciences and Chemistry oriented to atomic energy applications the emphasis we have detected is more on empirical advance than on theoretical insight.

D. Nobel Prize Profiles in SCIENCE

In Appendix G of the Interim Report, we speculated on "Other Literature" that attempts to characterize scientific progress. Our emphasis on textual clues in this report suggested a reconsideration of our earlier ruminations. To recapitulate (with embellishments), Richard Holm, a member of the Sloan Chemistry Program Committee, told us in a phone interview that "everybody grasps" the really important scientific advances. One way of "grasping" award-winning work, we reasoned, is by examining texts which explain their significance to a wide audience.

We undertook an exploratory analysis of the brief third-party descriptions of Nobel Prize winners that are published annually in SCIENCE. These are usually written by peers who know both the laureate and his/her work quite well. For the analysis, we retrieved the profiles for 14 Chemistry laureates (1974-84) and read them looking for:

* the skills (substantive areas of knowledge and techniques) brought to bear in the research;

* the "research trail" followed; and

* the implications of the work that help make it Prize-worthy.

Paul Samuelson, in describing the 1984 Nobel Prize in Economics awarded to Sir Richard Stone, comments: "Competent scientists, it is reported, usually have narrow interests. Brilliant scientists reputedly have broad interests." Our impression of the 14 Chemistry Nobel laureates is that they defy simple characterization. Some doggedly pursue a given problem or hypothesis for decades. In the process, they may acquire new techniques, but their focus remains unswerving. Others appear to be gadflies interested in many issues and willing to use various techniques.

Our tally of the threads that bind recent Nobel chemists' research into a pattern shows that seven exhibit a decided core area of interest, while four employ a core technique relevant to diverse problems. In terms of theory and experimentation, we perceived only one laureate chemist as a theorist alone. Six iterated between a new theoretical contribution and subsequent experimentation to test it. The other seven seemed primarily experimental. In all, the experimental bent of the Nobel chemists--five specializing in biochemistry, three in organic, and the rest in multiple or hybrid areas (e.g., organometallics, polymers, protein structure)--was unmistakable.

The implications of the Prize-winning research most often cited were its far-reaching impacts. In two-thirds of the cases cross-disciplinary implications surfaced. For instance, research based on the physical chemistry of macromolecules was linked to industrial synthetic rubber processes, inorganic hydrous oxides, studies of water structure, and antigen-antibody reactions. Whether the genesis of the work was esoteric or practical, the implications were fruitful for different audiences and purposes. This lends support to Holm's opening comment about "grasping" important contributions.

The convergence of knowledge to formulate Prize-winning research is itself variable in this select sample. Three of the chemists applied skills from one area to a fresh problem area; two others were so fluid in their intellectual interests and skill acquisition that they seemed to "random walk" wherever opportunities led. The majority, however, stuck with a substantive domain, accumulating skills as necessary. Sometimes, knowledge and techniques were embodied by an individual; in other cases, extensive collaborations within a lab evolved, then reorganized to meet the needs of the next interest.

In sum, the third-party Nobel profiles are written at a suitably understandable level. While some relate tales of unappreciated theories en route to the production of significant findings, these congratulatory descriptions are convincing about the merits of the research. We still do not know, however, whether the claimed breadth of impacts are reflected in citation patterns (both amount and dispersion).

A future agenda should also include a comparison of skills for "award-winning" and "ordinary" scientists. Furthermore, comparisons among award-winners--Sloan Fellows, Lawrence Awardees, and Nobelists--would temporize research performance, evaluation, and recognition at different career stages. Finally, we would hypothesize that (1) breadth of research implications would be relatively slow to develop, whereas (2) patterns of research skill acquisition might be set early (e.g., the willingness to tackle problems in areas other than one's main skill training; involvement in cross-disciplinary collaboration; intermittent v. sustained funding; and frequency of movement between institutions). How early and in what ways, in other words, does the adventurous research style manifest itself?

E. Conclusions

These diverse third-party texts convey the richness of qualitative insight inherent in thoughtful peer evaluations. It is instructive that scientists continue to place faith in their peers (and competitors), rather than in quantitative formulas, to determine who shall receive coveted awards.

We are tempted to generalize about the results of the Sloan and Lawrence letter categorizations: (O)bservation v. (T)echnique v. (S)cheme and combinations thereof. In the Life Sciences and Neurosciences, the rank-order of emphasis is OTS, in Chemistry, TOS. Nobel laureates in Chemistry seem to have contributed through Schematic interpretation of Observations obtained. This might reflect career maturation; it takes time to complete the "big picture" of a research problem. Sloan Fellows tend to demonstrate cleverness in T or diligence in O; Lawrence Awardees are said to persist in iterative efforts combining T and O; finally, Nobelists move through mid-career tapping T and O into Schematic advance.

Contrary to public images of science, third-party appraisals cite dogged empirical efforts and de-emphasize the role of theory-building in scientific progress. The "great men" of science, especially in chemistry, are no longer exclusively great synthesizers. Prize-worthy contributions come in many forms.

III. FIRST-PERSON EVALUATION: THE INTERVIEW AS TEXT

The central task of Phase 2 was to gather oral testimony from scientists about their conceptions of progress in research. Bearing in mind the five Sloan Fellowship fields--the four above plus economics--and seeking as much breadth as possible, we sampled purposively across natural science, social science, and engineering disciplines. As elsewhere, we included a substantial number of chemists as a "base" field. We considered the interviews exploratory; they are not representative of disciplines, specialties, or sectors of employment. Rather, we hoped to use them heuristically to generate "functional grammars" and to augment the proxies derived from other texts.

The interviews were loosely structured by an interview guide (see Appendix E of the Interim Report). A typical "ice-breaker" was to ask the interviewees to discuss exciting contributions to their research specialties, however they chose to define them, during the last 5-10 years. This led to a conversation that took its own course, touching upon several themes triggered by talk of "progress." We would interject specific queries to help the researcher clarify or elaborate as appropriate. A variation on this approach was to interview two researchers at a time, drawn from the same or cognate specialties. Five such interviews were conducted (in the presence of both the PI and Co-PI), accounting for ten of the 25 scientists in the sample.

A collective biography of the sample reveals the following profile. In disciplinary breadth, the breakdown was as follows: chemistry/biochemistry (n=6), neuroscience (3), economics (3), operations research/computer science (3), social science (3). The rest were trained as engineers or physicists. All the interviewees except one hold the Ph.D. In addition, five are engaged in science policy, four in federal agencies. The other have university appointments. Estimated median age was around 40. Among the academics, most were tenured and had been at their present institution at least three years.

The interviews were conducted as far away as London, but also in Washington, DC; New York; Lexington, KY; Tucson; Denver; and Atlanta. Most took place in offices or over lunch, but some took place in moving cars en route to or from professional meetings. All interviews were audiotaped and transcribed.

We approached the interviews with mixed anticipation, the words of Sir Peter Medawar (1986) from his recent autobiography ringing in our ears:

> The lives of scientists, considered as Lives, almost always make dull reading.... It could hardly be otherwise. Academics can only seldom lead lives that are spacious or exciting in a worldly sense. They need laboratories or libraries in the company of other academics. Their work is in no way made deeper or more cogent by privation, distress or worldly buffetings. Their private lives may be unhappy, strangely mixed up or comic, but not in ways that tell us anything special about the nature or direction of their work. Academics lie outside the devastation area of the literary convention according to which the lives of artists and men of letters are intrinsically interesting, a source of cultural insight in themselves.

So braced, we have organized excerpts from the transcribed interviews under themes related to general remarks about progress. All quoted material is indented and single-spaced. Sometimes the discipline of the scientist will be attributed, if the information seems germane. The excerpts are arrayed so as to tell different stories simultaneously, i.e., as key passages in longer texts produced by the interview situation.

A. Themes of Progress

Review of the interview transcripts led to the identification of eight themes. Some recurred more than others, but all were inductively salient. That each appears in the context of discussing scientific progress is telling about the associations researchers make among precursors, conditions, and outcomes. Here in no particular order, then, are the cognate and perhaps component themes of "progress in general" in science: commercialization, theory, funding, ad hominem, literature, jargon, methodology, and instrumentation.

Progress comes in at least two varieties, the familiar "incremental" and the other kind ("eureka") that most researchers never experience. We begin with the latter.

> Eureka for me is when an unexpected piece of the puzzle appears and I can apply the logic involved in that piece to some area. For instance, if I were to figure out a particular reaction pathway, add an electrode and there's one particular part that I couldn't figure out. Then all of a sudden it came into place and knowing this new pathway I can predict how something reacts in vivo. That's eureka.

> A breakthrough came in miniaturization. A Ph.D. in psychology became very interested in neurochemistry. Seems like a natural marriage. There's obviously some kind of electron transfer that's fundamentally involved with thought processes and electrochemistry is involved in electron transfer. There ought to be a meeting ground. But how do you stick into the brain to monitor this transfer process? When you use an electrode the size of my thumb, the patient dies. So there's a clear need to come up with a way to study the process in vivo using new approaches.

Now we have electrodes that are 5-5 microns in diameter that easily go into the brain.
Economics is built on this concept of marginalism which is really a parent stage of rationality, that is, people make decisions by weighing pros and cons at a margin. Suppose that someone came up with a rather definitive study showing that people really don't make decisions that way.... That would shake us up.

... the final collapse of behaviorism as the guiding theme in North American psychology has led to a lot of revived interest in information processing models, thinking models, structures of cognition, which lends itself very nicely to the sorts of questions of interest to decision researchers.... [W]ork coming out of the '40s and '50s associated with folks like Herbert Simon... [was able] to bring to the choice models of economics some psychological realism around the notions of the boundedness of human rationality and the thought processes that we are capable of.... But there are no established paradigms, so no safe ways of doing most of this research. A lot of it has to be thought up and worked out on a very piecemeal basis.

The more familiar and some would say mundane proxies for progress relate more directly to themes such as funding and the literature. The contrast to the "quantum leap" may simply be the proximity of the researcher to a problem he/she has been immersed in for some time. The passage of time allows the development of perspectives that ongoing participation denies.

One of the things that the ideology of theoretical physics doesn't always allow for is the importance of synthetic work.... I came to appreciate that there was a particular type of intellectual capacity for putting together information from the very wide range of fields--not in necessarily a very novel way--but in the way that it was supposed to be put together and... indicating connections that were not always clear to people. I've always been aware that people don't see the whole of their subject.

Specialization represents some fractionating of knowledge. It's not fair to call that progress per se.... Is this simply a relabeling? Is something semantic or is there something intellectual going on?

... progress has gone on to some degree by acclamation. That is, the peer review system tells us this is new or this is not, so not only do you have the claim but you have confirmation of the claim, at least to the degree of consensus... Like everybody else, we have an intuitive sense of what's different and what we know. My own field of behavioral endocrinology is noted for non-progress and one of the reasons I left it is a tremendous sense of chasing your own tail... Progress is an increase, an increment, expanding the phenomena that you're concerned with--not just being able to do it fancier.

It's really hard to find new ideas in the economic literature that are of any consequence. It seems to me that we do an awful lot of crossing of t's and dotting of i's... Advances? The origination of hedonic price analysis in the automotive industry in 1939: that's one hell of a miserable place to have a neat idea originate...

In the experimental program, if you have a synthetic objective, and you make the molecule, that's results. A significant molecule? Something as new as possible, maybe not predicted by past experience.

Unquestionably the most important thing in the field about 15 years ago was the establishment of the fact that computational complexity meant something practical.... Suddenly we realized that complexity might be the unifying point of view for this morass of problems .... Computational complexity has done more than anything else to give a unifying perspective to these problems. They fall into natural hierarchies, are related by certain transformations, and in some sense are all manifestations of the same hard problem.

Results accumulate in the same way that a coral reef grows or sort of by accretion all over in little places and not going in any particular direction. There are some areas of the coral reef where the solidity and the sunlight and the nutrients are just right so that the growth is particularly rapid, but the coral is not going someplace in the sense that it has a destination.

I don't think there are very many big steps. I don't think they occur very often. If you look at this century, the structure of DNA is considered one of the biggest advances in biology and yet that had a lot of background behind it. A lot of people were working on it.... I believe it's a lot of these little steps and if you're clever, you put together these things and you make another step.

... in my research I see it as trial and error in specific areas, but you have certain working hypotheses... You design something, you make it and it works. And maybe this one works a little better than the other one. You say, aha!

Part of the trick in this business, I think, is to be able to round up the necessary skills. A lot of people know what the problems are and how you go about solving them. The question is having the resources and the skills.

Resources come in at least two forms: funding (an input to research) and literature (an output, but once added to the curriculum vitae, another indicator of performance and capability--the stuff that weighs heavily on peer reviewers as "track record"). The linkage of these resources is an allocation of credit that warrants future investment in the scientist and his/her research program. Progress is inevitably tied to the financial support that scientists covet.

1. Funding

NSF prides itself on capitalizing on scientific opportunity... [W]e see an inkling that we've learned something and if we fund he'll go further.... If we add $50,000 to X we'll push him over the line. All we want to do is push him over the line. So when somebody says, 'but now I know what line it is that we ought to be pushing against,' I say, 'Come back when you're pushing, not when you're ready to push.' Subtle, but it's an important difference.

... people will bend the way their proposals are written and done to match what the funding agency feels its objectives are...

... if you don't fit yourself into a major funding area, everything else [the research program] doesn't exist.... [T]hose of us who have been in it a long time manage to know how to adjust our own interests and measure progress within the funding situation.

... the NIH peer review system works well.... It was not made to allocate scarce resources; it was made to fairly allocate reasonable resources.

The one problem I have with PI support directly is that it becomes a perception game... and then it's keyed on pedigree. I would rather hear the evaluation come [on the basis of] what have you done lately in the realm of good science. We have colleagues on our faculty that graduated from the right institutions 20 years ago and... they stopped being good scientists 19 years ago. But because they're riding the crest of their pedigree, things come their way.

... we wrote a proposal to NSF to work on this problem and it was turned down on the grounds that it obviously couldn't be solved, who did we think we were anyway?

We are attempting to make it [zinc neurobiology] into a legitimate field. There is a lot of vested interest in it. Of course, if everyone thinks it's a hot area, then it is easier to get funding.

You have to have good science... On top of that, you have to--I don't want to say gimmick--but you have to have certain sex appeal. You have to have good English, good writing, something catchy about it to make it fundable.... There is also a certain stamina factor to the competition... One of our elder statesmen in our department... says you guys don't talk about science any more. You talk about how many proposals you've written, how many grants you get, and you sort of notch your gun...

... to help support the development of the SSC [Superconducting Super Collider], there's several physicists who have said to me, 'You know, this is the last laboratory which we will be able to build on earth. Future laboratories will have to be the quantum leap where the next generation will be such a huge thing that it will have to be based in outer space.'

If we are going to understand something about how the whole process of allocation takes place, we need to understand a great deal more about peer review... Claims that are made by scientists who are applying for resources and the response of their fellow scientists to those claims seem to me to be a very interesting and absolutely sinful element of that process...

In more ways than one, both the decisionmaker and the researcher-applicant concern themselves with "the literature." It is a yardstick for evaluation, a basis for claims, a bridge between input and output that lends itself to talk of progress.

2-4. Literature, Jargon, Theory

Three themes that converge, yet emphasize different faces of progress, are the scientific literature (both contributions to it and uses of it), jargon (as a demarcating mechanism), and theory (as a manifestation that explanation has moved from here to there). In our respondents' words:

> The review article can be one way of gauging progress.
>
> I don't want to get too cynical here, but contributing to the literature is to me a political act. There may not be any substance to what you're writing.
>
> I don't know if it's this way in other disciplines, but in economics things are very trendy. In labor economics, you rarely will see a paper that doesn't have the sample selection bias problem addressed in it. In the macro[-economics] literature, if it's not in vector order regressions you better have a good reason why not.
>
> Flexible manufacturing is the current stump to thump, so now all the people who used to do queuing theory are calling it 'flexible manufacturing.' People who are getting tired of overcrowding in, say, communication switching and networks are now calling their queuing theories flexible manufacturing instead of calling it communication networks in hope of getting better funding there. You can see that sort of migration of terminology just in... permutations of the jargon words like 'management information systems,' 'decision analysis' tools, or 'decision aids.' It's endless. But everybody [is] still doing basically the same thing, but they attempt to give a sense of progress ... by sort of upgrading the jargon slightly. My impression is that the money simply affects the 'sound' and not any fundamental direction of the field.
>
> My sense of pride forbids me to get in this mode where you turn out one forgettable paper every six months. I have tried very hard to only write papers that I can honestly recommend that somebody read.... I haven't always maintained that standard, however, under pressures of publication, but that's been my goal and I think I've largely done that.

> I had written this attempted theory in the broadest sense trying to pull together 'zounds' of data and I sent it to the Grand Dad [of the field]. He called me on the phone and says, 'It's a nice paper. I want you to know that nobody is going to pay attention to it and what will happen is that everybody will ask for it and read it and tell you they love it. But it will force them to change the way they do what they do, and therefore, they're going to ignore it." I got 700 reprint requests for this article! It may have been cited ten times, two of which are for the theory and the rest are for the review function. [How large was the community at that time?] About 1200 maximum, maybe less. [That's a funny story.] Yes. [And sad.] Theory is scary.

> Theory which is not stated in an empirical context is frequently inoperative theory; it's not good theory, it's good recreation.

> I can probably think of one, two, three, four papers of which I'm inordinately proud. I suppose if I had to choose only one, it would be the paper about the application of space-going curves to 'meals on wheels' and here's why I like that best of all: It's published in the least respectable journal in my field because it's not the theoretical journal.... [I]t reports a theoretical breakthrough that is beautiful and in a way that every mathematician will agree, it's profound theoretically... has all of the aesthetic components--I mean, it's spare, elegant--just enough solution for the right problem and it's ethically admirable ... [T]he theoretical ideas... are spelled out right there, so that anybody... would understand all of the big theoretical ideas without having to wade through all of the theorems.

5-3. Methodology and Instrumentation

The transition from theory to practice, especially applications that solve problems is a constellation of themes that denote progress.

> In terms of progress, it's an interaction between capability of instrumentation, between theory and just doing experiments with a lot of serendipity involved. You may work in an area for awhile and ... there's nothing exciting, it's kind of a dead end. Then suddenly a breakthrough will come from

> theory or some applications area or from instrumentation that will allow you to do completely different kinds of experiments that you could never have done before... [Chemist]

> ... instrumentation is involved hand in glove with any progress here... [T]here was absolutely no progress made in electrochemistry until the lead storage battery was developed.

> I am using the techniques of computer science to solve problems of industrial engineering.... I'm more interested in the methodology and if a problem can be solved by that methodology or suggest some new methodology to me, that makes the problem more interesting.... I'm rather opportunistic when it comes to finding problems.

> To know something is to compute it. If I give you the initial position of a space shuttle and ask where it's going to be in an hour, I'm asking you for some information. We need to know that it must be computed... Can we have an answer fast enough to do us any good? The type of computation that goes into real time control such as rocket guidance needs answers essentially instantly in order for them to be useful.

> The biggest developments are developments of a new class of reactions that have wide applicabilities...

7. Commercialization

The application of methods and instruments to recalcitrant problems can be seen as intellectually compelling, even altruistic. But the packaging and marketing of applications can also be profitable. Some researchers estimate the end-point of progress to be commercialization.

> That would be the biggest accomplishment... before I die or retire: one of these drugs--one of these chemotheraputic agents that I work on--would actually be a useful drug. That would be a major lifetime accomplishment.

> ... if I could come up with a drug to cure emphysema ... I no longer think that that's possible for an academic investigator--possible, but highly improbable.... I'm optimistic that somebody will come up with something, and I'll look back and say 'I con-

tributed to that,' but I won't be able to say 'That is my specific compound.'

> ... the way you make a name for yourself... is when software vendors pick it [your code] up and include it in their software packages. For example, take the SAS package that everybody uses. If they pick it up and put it in there, Lord knows, here comes fame and glory.

c. Ad Hominem

Research is an extraordinarily interpersonal activity. Interaction with others--peers, administrators, editors, funding agents--is inevitable at every stage of the knowledge production process. So, too, are the tensions created by interaction, particularly when so much of the negotiation is evaluative. One's work and one's ego are intimately bound. Often, the competition brings delays and other setbacks. Sometimes this engenders ad hominem attacks. More common is frustration with the research "system" itself that is seen to retard progress.

> How do you separate out the cronyism? How do you separate out perceptions? I know some people who will only reference [in proposals and papers] their own work or work of cronies. [So they deliberately omit other relevant work?] Oh, yes, certainly.

> I know some of these guys who work in small sample distributions that will make the stuff just as hard as they possibly can. And then dare you to read it. If that's the way they want to behave, it doesn't do a whole lot for progress in the science. You get egos, people are people regardless. I find just as many 5-year-olds in the university as I find out on the playground. [Economist]

> We wrote a proposal to NSF to work on this problem and it was turned down on the grounds that it obviously couldn't be solved, who did we think we were, anyway? ... Then we came up with an interesting argument which says basically that when these problems occur in the field and they're abstracted into the 'travelling salesman' problem, there was 10-15 percent error anyway. So solving to within 15 percent... is about as good as you're going to do. We claimed to have solved

> the problem. There are people using our method who adore us. There are a lot of people in the traditional fields who either hate us or don't understand what we've done and dismiss it out of hand.... Because we claim to have solved a problem which they claim can't be solved, they feel that we're cheating or distorting.
>
> There is an important category of people who try to do too large a problem and then you look through it and you can see that they have simply fudged a number of steps in the argument, they made assumptions about things not known, etc.

B. Reflections on the Themes

Reviewing transcripts of 25 scientists discoursing on various facets of "scientific progress" mires one in the richness of the issues and the connections among them. To extract eight themes, as we have done, is to distill and simplify. To condense these eight further into two is more perilous, but let us try. The two themes are familiar, and therefore alluring: the inputs and outputs of scientific progress.

Modern science does not fare well--or at all--without resources. In a "chicken and egg" analogy, one wonders if fast-moving research areas attract funds because of their rates of progress or vice versa. Most certainly, it is a combination of the two, as the discussion of "research missions" (Chubin, 1984) suggests. The converse offers room for policy speculation: might slow-moving areas (e.g., the social sciences) benefit greatly from an infusion of significant support?

Our interview texts attest to the adaptability of scientists. As funding fashions change, interests adjust, perhaps more in jargon than in substance, but they adjust nonetheless. We recognize this as research "styles."

> We have two images of style in scientific research. In one image, the scientist accepts risk in order to gain knowledge. In the other image, he acts carefully and responsibly to protect existing knowledge and to ensure standards in the acquisition of new knowledge. The growth of knowledge is deemed to be derived both from bold conjectures and from hard and cautious study of the facts. It is accepted that both adventurous new ideas and exigent standards are needed (Wettersten, 1986: 443).

"Adventurous" new capabilitities, whether instruments or theories, change the course of science. They enable completely different kinds of experiments to be performed. Cross-fertilization, e.g., bringing computer science techniques to bear on industrial engineering problems ("multi-skill, problem-focused research") promises excellent payoffs (Porter and Rossini, 1984). Unfortunately, peer pressures favor the maintenance of research thrusts and publication within, rather than across, research areas (Chubin et al., 1986). That which is institutionalized is most readily rewarded. Research may create a constituency, but extant disciplines, journals, professional societies, and funding programs define ready-made constituencies. A challenge to science policy is to find ways to expedite and stretch cross-disciplinary dissemination of new conceptual schemes and techniques.

Scientists experience the quests for research support and for publication as highly interpersonal processes. Contributing to the literature does not guarantee contribution to scientific progress. Important developments may be overlooked or ignored in the marketplace of ideas. Research programs ("trails") develop significant inertia (Chubin and Connolly, 1982). Why this inertia seems so much greater in certain "backwater" locales than in other "frontier" areas, and what might be done to snap the "undue persistence" that blocks progress (qua innovation), are important policy questions. A sense of slow progress may well become institutionalized in the form of old boy peers, stagnant skill sets, static puzzles, and publication orthodoxy. Perhaps carefully targeted funding programs could leverage change, e.g., neuroscience initiatives which altered anatomy research programs.

What about change itself? The expressions of our scientists appeal to incremental advance as the typical mode of progress: an experimental result, accumulation of results ("like a coral reef grows"), and a new emergent synthesis. Yet the interview texts allow for the exciting "Eureka" possibility: a breakthrough solution, a reconfigured theory, a brand new line of experimentation. Quite insightful is the observation that the difference between increment and breakthrough may be a function of vantage point: distance (and time) may be needed to see the full implications of change or of an accumulation of little changes. Looking backward, we may all be Thomas Kuhns; looking forward, in the labs and at the benches of our respective specialties, is a more daunting enterprise. The myopia of the here and now produces blurred visions of progress. The glare of urgency and competition at the research front leads us to a methodological injunction: multiple accounts must be taken, sorted, and compared.

IV. CONCLUSIONS AND PROSPECTS

A. A Policy Perspective on Scientific Texts

This report, and indeed the entire project, has sought to develop novel ways of measuring science output. In Phase 2, we have particularly explored qualitative, subjective, first-person methods for measuring "progress" as an output of science. The question now remains: Is there anything in the methods we have employed or the findings derived from them that recommend usage for policy purposes? Put another way, is there an evaluative component in the analysis of scientific texts that complements conventional quantitative indicators of scientific performance, e.g., publications, citations, and awards received?

Our response to these questions can be framed by the words of recent commentators on the social analysis of science:

> what would a practical piece of sociological explanation look like? In the first place it would be fundamentally prosopographical: one would search for statistical correlations between the statistical correlations between the social circumstances of groups and their scientific beliefs ... (Shapin, 1982: 195).

> The social character of scientific work and findings is reflected in the mirror of technical competence. Social scientists who wish to observe it need 'a detailed familiarity' with specific laboratory techniques and matters of scientific knowledge that may be quite foreign to their intellectual bent. The drama of the sociologist in a laboratory is this inequality in the knowledge system: the observers' knowledge does not in itself make them capable of understanding that of the unobserved.... There is a gap of competence between the knowledge of the social observer and that of the laboratory members... (Polanco, 1986: 544).

The dilemma confronted by this project has been the "drama" of the social analyst, first to understand what scientists mean by progress and then to draw some "practical explanation" from this understanding. To accomplish this, the challenge of the "functional grammar"--methodological, terminological, and interpretive--employed by practitioners must be met. The shared meanings and usage by practitioners is what makes their study of prosopographical interest. Collective biography--be it of Sloan Fellows, members of a research specialty, or U.S. chemists awarded the Ph.D. in 1974--locates shared "scientific beliefs" in common experience and culture.

Lest we appear to rejoice in collective biography and the analysis of texts produced by and about the collectivity in question, we return soberly to the overriding concern for measuring progress. To invoke the phrase above, have we reduced the "gap of competence"?

Note that heretofore we have not contested the claim that progress occurs in a science. A decisionmaker often cannot afford this luxury. Scarce resources must be awarded on the basis of investigators' history of performance and competing claims of ability to produce and recognize new knowledge. Instead of disputing the investigators' claims, we have taken them as an empirical point of departure: how do they phrase them, what are their referents, what are the factors cited as necessary or inconsequential for progress to continue? Above all, how do investigators differ in their characterizations (or functional grammars)--among disciplines, research communities, and other organizational units they deem significant?

Fundamental as these questions may be, they pale next to those associated with national priorities, resources, and policies--yet are bound to them. For how might a decisionmaker intervene to accelerate progress, or alternatively, sound the death-knell for a research area lacking in the trappings of progress? What models of output guide such decisions (Kochen, 1976)? These are functional questions; they turn on assessment, evaluation, criticism, and review, and other euphemisms for the justifying the allocation and withdrawal of material support. They shift us from a literature on modes of discourse and their analysis to another on "knowledge utilization" (Dunn, 1983). As our purposes change, so do our categories and criteria of usefulness. This is well known at federal agencies that fund research (e.g., NSF, 1979). As the capstone to the Final Report, we confront this shift now.

B. Policy Uses of Project Findings

The unusual circumstance of departing in Phase 2 from the bibliometric approach of Phase 1, founded on the recommendation of an ad hoc panel of OUR peers, created less continuity of findings in this project than we hoped deliver. Our charge was to abandon the bibliometrics and explore content analysis through what we came to call "third-party" and "first-person" texts. The need to bridge these different kinds of data remains, and the analysis of "most-cited" and "best" papers for the 1974 and 1984 cohorts of Sloan Fellows in chemistry is in preparation (see Appendix B).

Phase 2, then, featured a methodological shift: we were asked to extend collective biography solely to the written and oral texts of scientists and engineers. The population was defined by awards for which they were nominated and less spectacular, albeit noteworthy, research contributions that earned serious scrutiny from peers, competitors, and decisionmakers alike. We mined this rich lode of conceptual data and found:

1. "Progress" takes on various methodological, terminological, and interpretive meanings depending on the discipline, the research problem, and the expectations of those interrogated. It is operationally useless to say that notions of progress are field-specific. They are far more specific than that and continually in flux. Progress is part of the "production cycle of knowledge": it is redefined as essential, attainable, or elusive as production evolves. This is a process, neither a single product nor a constant state. Thus, central to the culture of a science is an ongoing evaluation of what it takes to be progressive.

2. "Technique" is an ascendant or dominant characteristic of contemporary science. Scientists identify progress with instruments, methodology, and experimental protocol. From the practitioner's perspective, research advances through observation and results. Theory is a minority pursuit; schemes, solutions, and models are not to be denigrated, but they are the stuff of quantum leaps, and therefore, outside the realm of workaday scientific experience. There is a necessary division of labor in research. Appropriate skills are seldom embodied in an

individual; skill mix is the province of the team. Besides, great syntheses do not emanate, our respondents imply, from the investigations of the rank-and-file. Incremental advance is the modus operandi--and the kind of cumulation to which most contribute.

3. If there is a dichotomy between those who do the routine work and those responsible for the extraordinary, then do award-winners possess an "adventurous" style--fully apart from the organizational environment they find themselves in? This is perhaps a question in the hopelessly entangled "nature-nurture" genre. How much do careers depend on ability and how much on the accumulative advantage of an elite PhD institution, a visible mentor, and early opportunities (e.g., entry-level position and research grant)? This is an important vexing question because progress is seen as the sparks that fly BETWEEN researchers. It may also explain the growth of apparently interdisciplinary research areas. (For example, who does "surface science" and how does physical chemistry differ from chemical physics?)

4. The method of microscopic analysis which we employed--predicated on the selection of a small sample of texts (reference letters, interviewees, public evaluations)--allows for translation of scientists' discourse into the grammar of social analysis. Is this substituting one unknown for another? We think not. Oral testimony is particularly heuristic in generating "functional grammars." This means that our abstraction "progress" in placed in the researcher's context of themes that, although not synonymous with progress, are seen in various combinations as components of progress. These components seem to cluster under

the following labels: funding, literature-jargon-theory, methods and instruments, commercialization, and a residual "person" rather than "thing" category of ad hominem.

5. Amidst this overwhelming finding of diversity, what can we offer as a unifying thread? Certainly, Sloan Fellows, Lawrence Awardees, and Nobel laureates in chemistry are slices of the upper echelon of science; they are the successes. They are the easiest to define, retrieve information about, and gain access to. Because they are productive in research, they are in touch with the influences that make or break programs. They see science as a system with inputs, outputs, and necessary intervening activity. They are the population with whom decisionmakers interact or which they monitor. They are, in short, the symbol of the future of U.S. research. For balance, it would seem that we need analysis of other populations, e.g., those involved with what is retrospectively recognized as definitive failures or profound disappointments, areas of significant and sustained investment which produced too little or just plain unusable knowledge. Perhaps from such analysis, tell-tale signs of "what went wrong" can be developed and codified as a guide to future decisionmaking.

C. A Final Note

Science policy is itself an ongoing process of research evaluation. How is the best distinguished and supported? Social analyses of science now provide claims to assist in the evaluation task. Our claims, based on this project, may have assumed the flavor of caveats. We intend them more as "do's" than "don'ts." In summary, we offer two recommendations:

1. Quantitative methodologies require augmentation by other data. This is no revelation to the decisionmaker. But we contend that the textual data of Phase 2 can clarify and enrich citation data (ours in Phase 1 and most everyone else's) for evaluation purposes. The "audience problem" in citation analysis persists: it is not how many citations a paper or scientist receives, but the breadth or dispersion of those citations. Who is reading and drawing on the published research? Our respondents insist that bibliometric indicators do not gauge intellectual impacts. We simultaneously wish to agree and disagree. While we eschew simple formulas, we would like to devise a tool that incorporates peer judgment without deferring to it blindly.

2. The caveat concerning quantitative data has a positive side: let's keep talking to scientists. Unobtrusive, archival data sources have a vital role in evaluation. So do a range of relevant actors--on or off the record. We have evidence that scientists do not write as they speak, and may not act as they claim in accounts both to peers and non-specialists. Retaining skepticism is a sound methodological stance. In-person interviewing can be an unparalleled strategy in terms of candor, detail, and intelligibility. We urge continued interaction, between scientists and social analysts, including exchanges over OUR categorization of THEIR exposition. Narrowing the terminological gap between functional grammars renders tacit knowledge explicit. That which is explicit is more amenable to evaluation.

Decisionmakers should value that. We have certainly learned to do so.

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Roundtable

Scientific progress: an interim report

For the last year or so, I have been asking scientists what they mean by scientific progress. How do they recognize it? How do they characterize it? What I have learned from these interviews should not have surprised me, but it has. Although the ideology of science bills progress as its most important product, the interplay of theory and experiment that grinds toward new knowledge is not easily conveyed to the nonspecialist such as myself. What happens at the bench is a shorthand or vernacular; to describe it to one not steeped in the language is like trying to communicate with a sighted person via braille.

How do scientists trained to communicate with fellow specialists make their craft intelligible to others, even the science-literate, who do not share their background or specialized knowledge? This is a transparent problem that my interviews amply documented. The words I used in asking about so-called advances, breakthroughs, and incremental gains seemed foreign to those with active research programs. Some of these scientists were recent Sloan Foundation Research Fellows. Their considerable reputation and published output had already endowed them with promise, if not a halo of brilliance. The other interviewees, older but no less articulate, also had difficulty in explaining how they were making progress—what the signs were and whether they could be sure those signs constituted significant evidence that their research programs were adding to the stock of knowledge.

It is too soon to generalize from my interviews. Out of the diversity of specialties I've sampled—including organic chemistry, solid state physics, brain physiology, operations research, econometrics—patterns have begun to emerge. The interviews tie progress to a complex of relations and things—lab equipment, postdocs, project funding, peer review, journal policy, tenure. In this interim state, I had best opt for the words of the scientists themselves:

> Those who have been in it a long time manage to know how to adjust our own interests and measure progress within the funding situation. . . .So a lot of this is determined by the government.

> To know something is to compute it.

> "Eureka!" for me is when an unexpected piece of the puzzle appears, and I can apply the logic involved in that piece to some area—for instance, if I were to figure out a particular reaction pathway, add an electrode, and have one particular part that I couldn't figure out come into place all of a sudden. Knowing this new pathway I can then predict how something reacts in vivo. That's eureka. [Has it happened to you?] Yeah, two or three times.

> Your own work always turns up some new things that were completely unsuspected. This is why you cannot program science. The idea of being able to write up and do a research proposal is complete nonsense; it should not even be required.

> Tenure . . . tends to trivialize research. I know a great young chemist who was very, very original. Therefore, not many of his experiments would work. He had trouble getting tenure at one of America's prestigious schools, which is just the opposite of the way it should be.

Scientific progress is such an abstraction that its explicit expression is a challenge

From these snippets alone, one can sense that scientific progress is such an abstraction—a concept and value deeply embedded in the psyche of the scientist—that to extract from its tacit knowledge base (Polanyi 1966) and give it explicit expression is a challenge most scientists would rather decline. Scientists do not talk (or think?) in terms of progress; they speak of novelty, originality, elegance, accuracy, rigor, insight, solution, prediction. These are the words that analysts such as myself hear and can conceptualize. But among themselves, specialists converse in other terms and signal their assessments in ways imperceptible to the nonspecialist.

Yet no upstanding historians or sociologists of science will concede this lack of understanding as an insurmountable methodological barrier to their research. As Thomas Kuhn, who forever altered our idea of progress with his writings of the past two decades (1962, 1977), recently observed, "we simply no longer have any useful notion of how science evolves, or what scientific progress is." These are sobering words indeed. They illuminate a gap between what scientists say and do, and, moreover, what they say they do (e.g., Latour and Woolgar 1979).

I am glad I asked about scientific progress. It is clear, however, that what specialists perceive as bold and imaginative very much resides in the private domain of their own minds or in the semiprivate enclaves of their offices and laboratories. We may never access these perceptions, not because scientists will not share them, but because they *cannot*. They can't tell us what is progressive in their science because we don't have the language to understand their con-

by Daryl E. Chubin

cepts. And they don't have the patience to translate.

Finally, I suspect we may all be denying a bit our capacity to understand because that gloriously rational enterprise known as research may take measured steps due as much to the subconscious (e.g., Seltzer 1985) and the irrational as to the disciplined creativity that spearheads the ideology of science. I've got to talk to a few neuroscientists about that—before I submit my final report. Anything for progress.

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Daryl E. Chubin is contributing editor of BioScience.

APPENDIX B

Paper in Preparation

Porter, A.L. and D.E. Chubin, "Sloan Fellows in Chemistry: A Cohort Comparison of Career Research Output."

Abstract:

A bibliometric analysis of publication and citation performance of chemists awarded Sloan Foundation Fellowships in 1974 (n=28) and 1984 (n=23) is presented. The analysis includes a comparison of papers nominated as "best" by the authors themselves to papers "most cited" as determined by SCIENCE CITATION INDEX tallies. The strengths and limitations of bibliometric data as outputs of the research process are evaluated relative to the stage of authors' careers. The methodological implications of augmenting bibliometric data with first-person accounts of the intended audience for and significance of nominated papers are discussed.